



Early Journal Content on JSTOR, Free to Anyone in the World

This article is one of nearly 500,000 scholarly works digitized and made freely available to everyone in the world by JSTOR.

Known as the Early Journal Content, this set of works include research articles, news, letters, and other writings published in more than 200 of the oldest leading academic journals. The works date from the mid-seventeenth to the early twentieth centuries.

We encourage people to read and share the Early Journal Content openly and to tell others that this resource exists. People may post this content online or redistribute in any way for non-commercial purposes.

Read more about Early Journal Content at <http://about.jstor.org/participate-jstor/individuals/early-journal-content>.

JSTOR is a digital library of academic journals, books, and primary source objects. JSTOR helps people discover, use, and build upon a wide range of content through a powerful research and teaching platform, and preserves this content for future generations. JSTOR is part of ITHAKA, a not-for-profit organization that also includes Ithaka S+R and Portico. For more information about JSTOR, please contact support@jstor.org.

11. Ohio Effigy-pipes:—Professor Wiener here again employs both *suppressio veri* and *suggestio falsi* with a master hand. The question at issue is whether or not certain pipes found in Ohio represent the manatee and the toucan, animals and birds whose nearest habitat was in the Gulf of Mexico and the regions south. In order to give the impression that Henshaw's criticisms do not invalidate but rather confirm the accuracy of Squier and Davis' identification, Professor Wiener carefully quotes Henshaw's *second* conclusion, and then states that the latter thus "leaves the subject wide open." He deliberately *omits*, however, to give Henshaw's *first* conclusion which immediately precedes and which reads "that of the carvings from the mounds which can be identified there are no representatives of birds or animals not indigenous to the Mississippi Valley." In the body of his article, Henshaw has just shown conclusively that Squier and Davis' identifications of "manatee" and "toucan" pipes are absolutely untenable!

12. Teotihuacan "negroid" heads:—The word "post-Columbian" is to be sure not used in connection with these heads—but the entire paragraph in which they are mentioned, has for its primary purpose to suggest that the heads are "negroid" because they were made subsequent to the introduction of Negro slaves.

Professor Wiener's reply constitutes neither fair criticism nor legitimate argument; it is rather an evasion of the issues and a specious presentation of irrelevant or misleading facts.

ROLAND B. DIXON.

HARVARD UNIVERSITY.

THE REINDEER

MY "Notes on Reindeer Nomadism" (*Memoirs of the American Anthropological Association*, Vol. VI, 2) has called forth a reply from Dr. Laufer in the *Anthropologist's* April-June number, 1920, which has just come to hand.

I am heartily in accord with Dr. Laufer when he says "that facts mean everything." Facts should, however, be rightly understood; therefore, we must attempt to find out the right order in which the facts should be arranged so as to be fully comprehended. That is where theory comes in. Now, Eduard Hahn and after him Dr. Laufer have not, in my opinion, arranged the facts in the right order; therefore, I have tried to arrange them better. From Dr. Laufer's reply I learn of his disapproval of my analysis of reindeer nomadism and my attempt at a chronological stratification of the elements contained in this culture-

form. "The criteria made out for earlier and later phenomena are purely subjective and a matter of debatable opinion." Unfortunately, Dr. Laufer does not say why he regards my criteria as purely subjective. It would have been interesting to know why Dr. Laufer regards the geographical distribution of the elements of reindeer nomadism as purely subjective. The fact that he finds my work deserving of condemnation is not sufficient basis for a discussion.

When I find reason to suppose that Ørjan Olsen may have made a mistake regarding the taming of wild reindeer among the Soyot, I am not moved by any fear of having my theories contradicted or my "dreams" shattered. If Olsen's observations should happen to be perfectly correct, this would not seriously impair any of my theories. I find, however, that the process of taming, described by Olsen, has a suspicious similarity to the breaking of domesticated (not wild) reindeer among the Lapp; and it seems to me very curious that the Soyot should carry on a regular domestication of wild reindeer, as long as no such procedure is known from other reindeer tribes.

On a number of points I have criticised Dr. Laufer's paper, "The reindeer and its domestication." Some of these points he passes by in silence in his reply—*e.g.*, my criticism of his note on the Lappish and Samoyed sledges. Other points he takes up for discussion. With regard to Dr. Laufer's statement that "reindeer milk is not made into any product in northern Asia," I cited two instances to the contrary; Dr. Laufer declares that does not alter his views. That may be. More serious is his maintenance of his remarkable interpretation of Othere's account. Dr. Laufer declares that he does not read more into documents than is warranted by their contents. However, he not only refuses to accept the interpretation which our best authorities so far have given of Othere's reindeer account; he actually reads into this account something which it does not contain, and which is moreover in perfect discord with what we know of the culture of the old Norsemen and of the habits of the reindeer of the region where Othere lived. Othere's account does not state that his reindeer were "the venture of a sportsman, who had an aesthetic pleasure in the animals, like a park-owner in fallow deer": in this interpretation, Dr. Laufer goes entirely beyond the content of our document. Furthermore, the saga-literature, which tells us a great deal about the life and culture of the old Northmen, although not much about those of other nations, does not contain anything about deer-parks (one of our best authorities on the culture of the saga-period, Professor Valtyr Gudmundsson of the University of Copenhagen, has

verified that). And, what is also a serious objection to Laufer's interpretation, the reindeer of the region are strictly migratory. Dr. Laufer finds these two objections not valid; it seems to me—and to other students of Lappish ethnology—that they have considerable force. The refusal of Dr. Laufer to take account of them cannot be given much importance as long as he has not produced a single piece of evidence in behalf of his theory about Othere's supposed deer-park.

I am glad to learn that Dr. Laufer entertains no doubt as to the nationality of Othere's Finn. Page 95 of his paper gave me the impression that he was in doubt. Now, as Finn in Othere's narrative means Lapp, we learn from Othere that the Lapp in Othere's country in the ninth century caught wild reindeer by means of decoy deer—that is, they used tame reindeer in hunting, probably by the same hunting methods as those which were fully described by later authors on Lapland, which have been in use by reindeer nomads all over northern Eurasia. Othere does not say that he himself used his six decoy deer in hunting. He says, however, that he owned altogether a herd of six hundred tame deer. As Scandinavians are not known to have been reindeer breeders, except in a sort of cooperation with the nomadic Lapp who tended the reindeer of Scandinavian owners together with their own herds, and as Othere mentions the Lapp and their use of decoy deer at the same time as he speaks about his herd of six hundred, the inference can hardly be avoided that Othere, the Lapp, and the reindeer had something to do with each other, probably in much the same way as Scandinavian reindeer owners, nomadic Lapp, and reindeer in later centuries. If we are to understand Othere's reindeer account at all, we must read it in this way. Dr. Laufer's interpretation, on the other hand, is fanciful, as it does not agree with Othere's account, nor accord with other facts about Scandinavians and reindeer.

As I have shown (p. 125), an Icelandic poem, probably from the thirteenth century, mentions a Lapp chieftain, riding in a reindeer sledge. Dr. Laufer does not seem to have noticed that. On the other hand, he informs me of the undeniable truth, that Saxo's tale about Hotherus is legendary. I have, of course, not thought of doubting that; when I mentioned Saxo's tale, it was because an eminent folklorist has compared a passage in this tale with the reindeer-driving Lapp chieftain in the Icelandic saga and poem. As I have stated several times, we do not learn much about other nations in the sagas. I find it, however, natural to adduce what the sagas have to say on Lapp and reindeer.

Dr. Laufer's sweeping assertion, that "no historical facts should be

deduced from the status of loan-words and other linguistic phenomena" is an attack on the eminent philologists who have deduced historical and culture-historical facts from loan-words and other linguistic phenomena in Lappish and neighboring languages, and does really not concern me, as I am not a philologist nor am I claiming to be one. I have cited the opinion of recognized philologists upon Lappish reindeer nomadism, because I found it desirable to call attention to as much material as possible, bearing upon this problem. I would advise Dr. Laufer to study the particular works in question before passing his sentence. Comparative philologists have probably made many mistakes, especially in their attempts at reconstructing the original "Indogermanic" culture. I fear, however, that by condemning indiscriminately all philologic attempts at deducing culture-historical facts from linguistic phenomena, Dr. Laufer may perhaps strike at more heads than he can easily cut off.

Now we arrive at Kalevala. Unfortunately, Dr. Laufer revealed in his paper a fatal lack of understanding of the real culture-historical import of this remarkable epic. It was necessary, therefore, to state shortly what Kalevala is, according to modern folklore. It is not at all my own ideas I have set forth about Kalevala; I may, therefore, justly disclaim the honour of being ridiculed by Dr. Laufer as a self-constituted Kalevala authority. I would, however, advise Dr. Laufer to read his classics, and some modern authors too, a little more carefully, before he takes his final stand in the question of Kalevala's bearing upon reindeer nomadism.

When I maintain that the description of Lapland and the Lapps in Kalevala is not realism, I do not assert, of course, that Kalevala does not contain any glimmering of truth about the Lapps. It is, however, on the whole a distorted, fanciful, imperfect, unrealistic picture of Lappish culture that can be gained from Kalevala. Laufer cites a number of verses in which snowshoes, reindeer-hunting, and sledges drawn by horses are mentioned. I do not quite understand why he does that. Is he not aware that horse driving is a Finnish trait and never was a Lappish one, and that snowshoeing and elk and reindeer hunting are old Finnish sports?

Dr. Laufer has the audacity to assert, "Had it so happened that the Kalevala furnishes the opposite data which would support Hatt's presumptions, he would probably have accepted them without hesitation." This is glaring unfairness. If Dr. Laufer has read my paper through, he cannot have avoided noticing (p. 127) that I have cited a passage from Kalevala where the reindeer actually is mentioned as the

northern or Lappish equivalent of the horse. As I have said already in my paper, I do not quote this passage as proof of the antiquity of reindeer nomadism—which would, in my opinion, be entirely inappropriate—but solely to show that Dr. Laufer has not read Kalevala carefully enough. I confess that his reiterated assertion that Kalevala “does not contain the faintest allusion to domesticated reindeer,” is evidence of a steadfast mind.

The value of a discussion depends upon the validity of the arguments which are set forth, a validity notably lacking in Dr. Laufer's recent criticism.

GUDMUND HATT.

COPENHAGEN,

October 16, 1920.

WHO WERE THE PADOUCA?

Dr. Grinnell, *American Anthropologist*, Vol. 22 (N.S.), p.248 *et seq.* discusses the question as to who the Padouca were, and states (p. 260) that “the evidence . . . convinces me that the Padouca were not the Comanche, and I am disposed to regard them as Apache.” Without wishing to review his entire article, I may point out that the Foxes call the Comanche and no other people Pātō'kā^a: see William Jones, *Fox Texts* [1907], p. 216; and this is substantiated by my own information. It is obvious that this has an important bearing on who the Padouca were.¹

TRUMAN MICHELSON.

BUREAU OF AMERICAN ETHNOLOGY,

WASHINGTON, D. C.

¹ Naturally Pātō'kā^a is not in the synonymy under the article Comanche, *Handbook of American Indians*, but Dr. Grinnell has apparently ignored the fact that other living Indian tribes also know the Comanche by equivalents of “Padouca”: see the synonymy under the article Comanche in the said Handbook.